H-Diplo Roundtables

Bruce Kuklick. Blind Oracles: Intellectuals and War from Kennan to Kissinger. Princeton University Press, 2006.

Roundtable Editor: Thomas Maddux

Reviewers: Robert Jervis, Charles Maier, Anders Stephanson, Jeremi Suri.

Author's Response by Bruce Kuklick

Right at the start I would like to express my gratitude to H-Diplo for sponsoring this Roundtable, to Thomas Maddux for editing it, and to the four scholars who have made a substantial effort to come to grips with the book.

The authors agree,[1] and I agree with them, that the book is idiosyncratic. I wanted to steer between political science and history, and between diplomatic and intellectual history, and I wanted to offer explicit commentary about the sorts of issues I was taking up. The basic notion was to look at the most influential men with claims to being intellectuals who had been involved with the making of foreign policy from 1945 to 1975. What were their ideas? What were the social sources of the ideas? How influential were the ideas in the actual determination of policy? What was the added value of America's commitment to a science of foreign policy making during the first phase of the Cold War?

It came as a surprise to me that my eventual answers to these questions and others like them were skeptical. Despite their best efforts, the men I studied did not know very much. Despite their attempts to secure scholarly detachment, they shared the prejudices and assumptions of non-scholars. Overall, they had a small causal role in deciding issues, and they can't be said to have improved policy, although they can't be blamed for making it worse either.

I am unconvinced that conventional political studies that expressed standard operationalist views in the university assisted in the creation of a better foreign policy in the United States from 1945 to 1975. But I am equally doubtful that those intellectuals hostile to standard social science had much better to offer. In my own head my most critical feelings are reserved for those people who most explicitly brought a canned or potted form of historical assessment into their efforts. My skepticism extends in all directions. At the same time, I offered limited alternatives to what went on in policy institutes and think tanks. I concluded that despite the pedestrian record, academics have to continue to do the sorts of things they have been doing.

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[1] I exempt Professor Stephanson, at least in part, from my comments because his essay is written at an elevated conceptual level that I had difficulty in understanding.

My reviewers perhaps share two beliefs. The first is that I am too negative in my estimation of what the intellectual diplomats achieved. The second is—Professor Maier puts it most clearly—that my views are "even inconsistent." For Maier especially there is a tension between my appraisals that the intellectuals were of no account and the respect that I have for some of these people, Richard Neustadt in particular. And how can I complain about the thinking of defense intellectuals if I reckon that they should carry on along the same lines today?

I would like to take up each of these two beliefs in turn.

Professor Maier is temperate in his criticism, and I take its point. But it does seem to me he does not give due attention to certain aspects of the book. One aspect is something else that surprised me. Henry Kissinger deserves some credit for making ideas count in foreign policy. I am not sure that they counted benignly, but they did count, and I did spend some time saying why.

The other aspect is that I do say again and again that even if policy intellectuals should continue in the same tradition, there needs to be one major alteration. Some way must be found to limit the arrogance and hubris that again and again I have seen expressed by men of mind in the corridors of power. My one positive belief is that we would all be better off if the intellectual establishment in foreign policy could have its conceit and self-importance curbed. Indeed, Professor Maier shares just my own belief about how the sort of history that professional historians promulgate can play a role in the policy world. Maier says history should make policy makers aware of "how much scope there is for caprice, contingency, /and/ miscalculation." And, Maier writes, at best "knowledge of history humbles the policy maker."

The second issue is my (almost) unremittingly critical evaluations. Professors Maier and Suri are historians accustomed to making balanced judgments. They are leery of lack of nuance. Professor Jervis is a distinguished political scientist, wedded to the principle that his discipline has constructive contributions to make to policy study. He has a built-in prejudice against accepting my analyses; acceptance would mean that the generation of policy scientists who nurtured him had labored in vain. And what would that say of the work of his generation?

All of these reviewers are more persuaded than I am of the possibility of usable historical and social scientific knowledge. They all have a fairly robust idea of the worth of scholarship, its benefits for the public, and (as important) the determinate nature of their findings. We all talk about the rise of various forms of cultural relativism in the last seventy years, but in practice we academics reject its assumptions when it comes to our opinions of what we know to be true about the past and present. While I want to stick to my guns, I appreciate the gentle reminders of Professors Suri and Maier that an eyebrow should be raised to such comprehensive negative evaluations as mine are. Professor Jervis notes that I am rendering "more pessimistic judgment about the possibilities of systematic knowledge than most of us are willing to accept." I think that is a correct statement, but I worry that my conclusions are unaccepted because they are pessimistic and not because they are unacceptable. Professor Suri writes: "Without the service of scholars, the past and future of American foreign policy would probably look worse." Here I worry about what he means by "probably."

A commitment to objectivism, again, is especially and predictably true of Professor Jervis. But it is also his critique that is most compelling to me, and I would like to spend some time engaging with it. Jervis offers a fine-grained account of foreign policy thinkers from the later 1940s to the early 1960s that he believes is something of an alternative to mine. I would barely dispute his history, but for the sake of argument I am also willing to accept his emendations. In his focus on the 1950s he makes two important calculations. First, he allows that RAND-connected people like Bernard Brodie, Albert Wohlstetter, and Thomas Schelling made a positive contribution to Cold War stability by their ruminations on mutual assured destruction. Second, in a closely associated argument, he suggests that this idea and related concepts were novel in the history of diplomatic thought. Both surmises are a way of intimating a helpful role for the political scientist.

I would not argue against assured destruction as a positive notion. What I did say, and would repeat, is that, especially on the part of Wohlstetter and his circle, the idea was one of a whole array of commitments. The RAND thinkers additionally presumed that the United States had to have an enormous military budget to procure an effective deterrent. The strategists overestimated the nuclear and thermo-nuclear capacity of the USSR and, crucially, postulated that the Soviets would use their weapons just out of sheer malevolence.

It is this dark and deductive grasp of the mental world of the Russians that has to be factored into any historical appreciation of the RAND group. It suffered from a trouble that I think Professor Jervis doesn't fully comprehend. The intellectuals had only a little tactile sense of the Russian position. They took their lead from what they discussed in the seminar room and their a priori theorizing—not a strange phenomenon when we consider that after all they had claims to be men of mind. We can't just extricate one element of their thought and declare it to be a plus; we've got to see it as part of a more suspect whole.

Professor Jervis's other argument is that his RAND mentors introduced new and useful concepts into our understanding of international relations. This argument is a powerful one, an insight of wisdom.

Alfred North Whitehead once said that all philosophy consisted in a series of footnotes to Plato. Even if this homage is true, there are footnotes, and Plato himself did advance his own (new) ideas. Most areas of inquiry admit to more progress than that made in philosophy, and it would be foolish to make global assertions about the lack of improvement in political studies and other forms of social understanding.

Professor Jervis's specific example is Wohlstetter's notion of a second-strike capacity that gained credence in the late 1950s. In the early 1950s, Jervis says, the policy makers are almost unintelligible in their discussions of how to attack the USSR, how to ward off an attack, and with what resources. Statesmen didn't really have a grasp of a defense guaranteeing that an attack by an enemy could never be so successful as to prevent a devastating retaliatory strike. Clear thinking about second-strike capacity *emerged* by the late 1950s as defense intellectuals were acclimated to and refined Wohlstetter's thinking. These ideas then made their ways into governmental circles. Jervis makes an excellent case that this is one way the international relations community works at its best. Scholars do advance novel concepts, and they come to

permeate our understanding and sometimes become the conventional wisdom, and sometimes are good ideas. It is a serious error to assume the ideas don't have a history—they come from somewhere, from the creative mind of the best of the professoriate.

I feel the force of this argument, and while I don't think *Blind Oracles* denies it, I should explicitly say in this response that I accept what I take to be Professor Jervis's general view about the history of practical (and impractical) ideas. Again, however, I am not so sanguine as Professor Jervis. I am less certain of progress, and less convinced than he is that international relations theory is an area in which there is much steady advance. In respect to Jervis's example, I am not satisfied that Wohlstetter's work is so astute. I wrote in the book (pp.50-51) that Eisenhower had some sense of RAND ideas, and I don't find the national security council's deliberations in the early 1950s as opaque as does Professor Jervis. But he may be correct.

There is a gap between Professor Jervis's estimate of the efficacy of political science scholarship and mine. Even so, I would not focus on this feature of our disagreements. As with Professor Maier, I would underscore my view that even if political scientists in policy roles have done better than I think, they still ought to have behaved with greater modesty and caution. It is one thing for us to preen before undergraduates, or to play God with graduate students. It is quite another thing to bring the habits of the classroom or the university into a political arena where we are unaccountable and where many lives may be lost.

Professor Jervis, indirectly, seems to me ultimately not to make a much stronger case. He writes that my intellectuals "were no more guilty of failing to understand the Soviet Union or to explore questions of whether the United States was acting for good or evil than were most contemporary scholars." In a similar manner, when I interviewed Carl Kaysen, an assistant to McGeorge Bundy in the 1960s, he made clear to me that the group of which he was a part did not do worse than other academics. This is faint praise! These were the guys who were supposed to know. These were the people the nation relied on. If we could generally substitute for them some professor in an English Department, or an anthropologist who studied the Zuni, what was the point of the expertise?

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